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GEOLOGY.—*Abstracts of papers on isostasy and related topics given before the Geological Society of Washington during the spring of 1930.*

I. SOME PROBLEMS OF MOUNTAIN STRUCTURE AND MOUNTAIN HISTORY.[1]

Chester R. Longwell

Attempts to establish some of the principles of tectonics call for critical examination of the concepts, old and new, which have gained a prominent place in this field. I propose to consider briefly three questions: (1) Is diastrophism periodic? (2) Do folding and thrusting result directly in mountain uplift? (3) What is the rôle of isostasy in relation to mountain making?

(1) Many geologists believe firmly that diastrophism is periodic and several modern textbooks offer this doctrine to the student without reservation or qualification. At best, the doctrine involves a loose definition of the word periodicity, which to the mathematician and physicist denotes a relationship that can be expressed by a rigid formula or represented by a regular curve. Events that seem to recur approximately with each *geologic period* can hardly meet the requirements of this definition, since the geologic periods probably differ greatly in length. Moreover the intensity of diastrophism appears to be distributed irregularly. A composite curve prepared by Holmes[2] has the required regularity, but the time period assumed is highly speculative, and to this is added the uncertainty as to the exact position in the geologic scale of many diastrophic events. Stille, after a com-

[1] Abstract of *Some problems of mountain structure and mountain history*, Am. Jour. Sci., 19: 419–434, 1930.

[2] HOLMES, A., *The Age of the Earth*, p. 49. Ernest Benn, Ltd., London, 1927.

prehensive tectonic study,[3] describes diastrophism as periodic; but he groups all post-Cambrian disturbances into three "orogenies," in each of which the separate events are disposed with some irregularity about a culminating point. This conclusion is expressed in graphic form by Lotze,[4] who attempts to represent the relative intensities of deformation. If anything approximating a periodic relationship of diastrophic events ever is established, probably it will be between a few widely-spaced high points, and not between events in successive geologic periods. At present, the most we can say is that diastrophism has been *recurrent*, and that a few episodes, widely-spaced, seem to have been of unusual severity. Periodicity may be entertained as a promising hypothesis, but the common assumption that this relationship is established is wholly unwarranted.

(2) Are genuine mountains made by folding and thrusting? Berry expresses the belief that the present Appalachians are the only considerable mountains that ever existed on their site.[5] Probably his reasoning is by analogy, since elsewhere in the same paper he points out that the present height of the Rockies, Andes, and other mountain units has been produced by vertical uplift much later than the folding. We should keep in mind, however, that the Appalachian folding is much older than the Rocky Mountain deformation, and that the Tertiary arching in the Appalachians occurred after a vast quantity of rock had been removed by erosion. Possibly this arching is merely the latest and weakest of several vertical uplifts that succeeded the Appalachian folding; and considerable initial elevation may have resulted from the folding directly.

For deductive study, consider the section in central Pennsylvania, where the folds are relatively simple. The Nittany Arch and other great folds, if reconstructed in any reasonable form, would rise nearly five miles above the truncated limbs. Assume that the land was at sea level when the folding began, and that isostatic equilibrium was maintained throughout the deformation. This would require that anticlines like the Nittany Arch rise to mountain heights, unless erosion proceeded as fast as uplift. But if the competent formations had been cut through at an early stage of the folding, then with further

[3] STILLE, H., *Grundfragen der vergleichenden Tektonik*. Gebrüder Borntraeger, Berlin, 1924.

[4] LOTZE, F., *Die Joly'sche Radioactivitätshypothese zur Erklärung der Gebirgesbildungen*. Nachrichten der Gesellschaft der Wissenschaften zu Göttingen, Math.-Phys. Klasse, p. 102, 1927.

[5] BERRY, E. W., *Shall we return to cataclysmal geology?* Am. Jour. Sci., 17: 1–12. 1929.

compression it seems that these stiff units would have been pushed over the eroded crests to form erosion thrusts. In central Pennsylvania, however, no such thrusts were formed. This argument favors the idea of considerable initial uplift, and is strengthened by the point that folding would concentrate rock of low density at the top of the zone, requiring a rise of the surface to maintain equilibrium.

The limited sedimentary evidence that has been preserved indicates high topography along the Appalachian axis in Triassic time. In the Rocky Mountain region the character and structure of early Tertiary sediments constitute strong evidence of high topography while folding and thrusting were in progress.

One of the most hopeful methods of attacking the problem consists in comparison of the older folded zones with those in which similar structure has been formed in late geologic and recent time. The Timor-Ceram island arc, in the East Indies, represents a large anticline which is being crowded against the Sahul shelf.[6] This fold is almost uneroded, and obviously its vertical growth has progressed with horizontal movement. The Alps may be taken to represent a later stage in the development of similar mountains. Apparently in the early stages the Alps were similar to the Timor and neighboring arcs. In the Miocene, after the greatest piling up of the thrust sheets, the present Alpine area was a range of considerable height, shedding coarse debris into the Molasse synclinal trough. The last pulse of compressive deformation, in the later Tertiary, was followed by general vertical uplift which completed the mountain growth.

Taken together, the evidence from the East Indies, the Alps, the Rockies, and the Appalachians suggests that folding and thrusting are attended by considerable direct uplift, but only a fractional part of that indicated by the crustal shortening, because approximate isostatic balance is maintained. Later there is strong vertical movement, carrying the mountain region to far greater height. Other pulses of uplift rejuvenate the wasting mountains, even after almost complete planation. In the Appalachians the latest vertical movement was by regional arching; in the Sierra Nevada area it was by tilting uplift of a great fault block. The existence of Cretaceous and early Tertiary highlands on the site of late Jurassic folding in the Sierra Nevada area is attested by vast quantities of sediments furnished by this belt during those periods.

[6] BROUWER, H. A., *The geology of the Netherlands East Indies*, Chap. 3. The Macmillan Co., New York.

(3) The relation of isostasy to mountain making has already been referred to in some of its aspects. Bowie and others have performed valuable service in persistent emphasis of the isostatic principle, but have failed to consider much of the geologic evidence bearing on the subject. Bowie represents the crust as too weak to bear even moderate vertical stresses, and incapable of transmitting horizontal thrust. The geologist sees overwhelming evidence of repeated deformation by horizontal movement, and bases his estimate of crustal strength chiefly on this geologic evidence. Some of the geodetic data also, interpreted from the geologic viewpoint, appear to support the conclusion of great crustal strength. One example is sufficient for illustration.

The work of Hayford and Bowie reduced the gravity anomalies in the United States to a small average value. However, isolated stations, and even some groups of stations covering large areas, still show anomalies of considerable size. Bowie has pointed out that many of these outstanding anomalies appear to be related to the local geology; positive values being explained by rocks of high density, and negative values by light rocks, near the station. We are familiar with David White's study of this question.[7] He found that all of the stations in the "Appalachian Valley" show pronounced negative anomalies, whereas most of the stations on the crystalline rocks just to the east show positive values. This distribution is striking, and the suggestion of control by the Appalachian structure is obvious. White concluded that part of the explanation for the negative group lies in the great depth of folded sediments; but since these formations are highly indurated and only slightly deficient in density, there must be another important factor also. He suggested that the crystalline basement under the sediments is also abnormally light as compared with the rocks in the Piedmont, because it has not been subject to erosion since the beginning of the Paleozoic, and therefore the denser rocks are still at great depth in the valley belt. This explanation assumes perfect isostasy, and seeks only a special local cause for the anomalies. The conclusion is heartily indorsed by Bowie.

Let us compare this case with another. In his study entitled "The strength of the earth's crust," Barrell attempted to show that great modern deltas are loads borne by the crust. A few years later Bowie studied the same problem, with the advantage of several gravity values recently determined on the Mississippi delta. There are eight

[7] *Gravity observations from the standpoint of the local geology:* Bull. Geol. Soc. Amer. 35: 207–278. 1924.

of the stations of which four show positive and the other four negative anomalies. Taking the group as a unit, the gravity anomaly is essentially zero; precisely, it is negative by only 0.007 dyne. Bowie closes his discussion of the group as follows: "We are evidently justified in concluding that the block of the isostatic shell directly under the Delta of the Mississippi is very nearly in isostatic equilibrium and that the delta material has been compensated for by a movement of material from the base of the block."[8]

The geologist, approaching the subject fresh from the discussion of local geology and its influence on gravity, naturally looks for some evaluation of this factor for the delta stations; but the subject is not even mentioned in Bowie's discussion. If there is any one area in which the influence of the underlying rock on gravity values should be carefully considered, surely that area is the Mississippi delta. On any reasonable premises, the depth of abnormally light sediments must be large. It is estimated that the thickness of the post-Paleozoic section in the Gulf region is of the order of 15,000 feet; it may be much thicker under the delta, depending on the amount of subsidence that has accompanied delta building.

Let us start with the assumption that the delta is in equilibrium. Taking the thickness of the sedimentary section as 15,000 feet and the deficiency in density as 0.4, and using the method of computation given by Bowie,[9] the average negative anomaly ought to be at least 0.060 dyne. Since this negative anomaly does not exist, the measured gravity is abnormally large, and therefore the delta area is overloaded. According to the table of ratios proposed by Barrell,[10] this anomaly corresponds to a thickness of 3,000 feet of rock. This estimate could be reduced considerably and still allow the conclusion that the crust is very strong even under vertical bending stresses.

This reconsideration of the delta anomalies suggests further examination of abnormal groups elsewhere. We do not feel justified in starting, as did White, with an assumption of perfect isostasy. Returning to the Appalachian trough, it seems more probable that a considerable part of the negative values indicates lack of adjustment in this area. As a result of the latest uplifts erosion has attacked the belts of weak sediments, and a large quantity of rock has been re-

[8] W. BOWIE, *Isostatic investigations and data for gravity stations in the United States established since 1915:* U. S. Coast and Geodetic Survey Special Pub. No. 99, 1924, pp. 49–50. Also in *Isostasy*, pp. 89–91.

[9] *Loc. cit.*, p. 37.

[10] BARRELL, J., *The strength of the earth's crust:* Jour. Geol., **22**: 309. 1914.

removed. The stations in the valley belt are located in these areas of maximum erosion. The physiography of the region indicates that uplift has occurred in a succession of pulses, with intervals of quiet between. It is highly improbable, therefore, that perfect adjustment has been maintained; and the large negative anomalies are in accord with this view.

Both the geologic and the geodetic evidence, then, lead us squarely back to the conclusion of Barrell that the crust is strong enough to bear loads of considerable size. This is the only ground on which all of the facts taken together will harmonize.

II. ISOSTASY: WHAT GRAVITY MEASUREMENTS REVEAL. *G. R. Putnam*

The results of gravity measurements afford abundant evidence that the crust of the earth is in some sort of equilibrium, but the interpretation of these results presents some difficulties because of the elaborate mathematics involved. A large part of the available data has been derived on an assumption of complete local isostasy, with the assertion that any error resulting from this assumption is negligible. This has been a source of perplexity to geologists. It has now been proved that gravity results derived on an assumption of complete local isostasy may be materially in error, as is rather clearly shown by the results in extremely mountainous or uneven regions. An assumption of a moderate degree of regional isostasy makes the gravity results more consistent within themselves, and also makes them more harmonious with reasonable geological theories and evidence.

The theory of isostasy holds true whether equilibrium exists between large surface blocks, as is probably the fact, or between very small areas. Thus far the only gravity reduction method which eliminates the excessive residuals for very high summits is a method based on an assumption that blocks of about 100 miles radius are in approximate equilibrium. This indicates that features of moderate extent, such as single mountains, are not separately compensated, but are supported, in part at least, by the strength of the earth's crust.

Regional isostasy is a conception of the earth's crust wholly different from that of perfect, or nearly complete local isostasy. It is in harmony with the known strength of the crustal materials, and with reasonable density distributions, with which local isostasy can not be fitted in. It permits the application of ideas of crustal action under loading and unloading, bending or breaking of the crust, difficult to conceive of with local isostasy.

The gravity results clearly prove the existence of a state of equilibrium in the earth's crust, and strongly indicate that this isostasy falls within limits which are not unreasonable from the geologist's point of view.

III. Some problems in isostasy. *R. W. Goranson*

In view of the many recent rather eulogistic discussions on the subject of isostasy I shall limit myself to a few questions that still await clarification. Isostasy can be made an important tool of geophysics—viz., as a criterion of crustal equilibrium in the continuous process of deposition and erosion with its consequent shifting of loads, but to do this our gravity anomalies must be reliable.

Now we have considerable evidence to show that the upper part of the crust tends to approach equilibrium discontinuously by faulting—e.g., the uplift resulting from the unloading of the Pleistocene ice caps is taking place by step-faulting, subsidence in the Hawaiian and Fijian Islands is accompanied by faulting; furthermore, discontinuity of subsidence is indicated by deposition cycles of geosynclines, which is just what we would be led to expect in a material of finite strength.

Hence, if we knew the deviation of a region from equilibrium—i.e., could evaluate the stresses and knew the strength of the crust—we might be able to supply answers, in some cases at least, to two outstanding questions: Are earthquakes apt to occur or continue in a certain region? If so, what will be their periodicity and intensity? Earthquake periodicities resulting from statistical studies do not answer these questions but, on the other hand, may be likened to the firing pin of a semi-diesel engine which although kept continually hot yet will not fire the charge until it attains a certain pressure.

The first question to arise is: How reliable are the gravity anomalies? If we assume the measured values to be good—i.e., within 0.003 dynes per cm^2, this question can be phrased as—What is the possible error in the computed value of gravity? It lies in the corrections for compensation and topography.

It is necessary, in calculating the amount of compensation, to assume a standard column as a comparison. In order to do this intelligently one must examine the data on variation of density with depth and again on how this varies with latitude and longitude.

Let us merely recapitulate what is generally conceded to be the situation, since time does not permit us to examine critically the reasons for these conclusions.

First, seismological data tell us that lateral heterogeneity seems to be limited to a depth of 60 km or less. Seismology has also given us a general picture of widespread lateral heterogeneities, as for instance between oceanic and continental columns. We know that a continental column to an average depth of 30 ± 10 km is of a "granitic" type and below this to a depth of 45 ± 10 km, of a "basaltic" type of rock. These depths vary with locality and seemingly bear a relation to topographic elevation. Under the Pacific, on the other hand, the upper granitic shell is apparently non-existent. Lying below 45 ± 10 km in both regions is a "peridotitic shell" in which density is apparently a function of depth only.

TABLE I

Anomalies for different types of compensation

Station elevation 787.5 m. av. for circle 170 km. radius

g (observed)	980.404	
γ_0	980.618	
Elev. correction	−.243	
Topog. correction	+.087	(to 170 km)
Column correction and distant zones	+.019	
	980.481	

Isostatic correction. = .077

Compensation	*Anomaly*
A. Pratt-Hayford (113.7 km)	−0.020
B. At 26 km depth	−0.004
C. At 50 km depth	−0.016
D. Divided, 26 and 50 km depth	−0.009
Max. diff.	0.016
Av. deviation	±0.006

At some depth we shall, presumably, reach plastic material and thus a depth at which inequalities of load have all been ironed out. Between this plastic material and the surface isostatic movements will take place largely by faulting and thus seismology should give us a clue to the depth of this plastic layer. The clue is that earthquake foci seem to be definitely centered in a region lying between the surface and 35 km depth. But even if faulting did extend much farther, the compensating deficiency of mass will lie at about 20 ± 10 km and

45 ± 10 km depths. If compensation extends below 50 km depth the center of gravity of the compensating deficiency will lie at about 35 ± 10 km depth. If compensation is complete above 50 km then the center of gravity of the compensating deficiency is at about 30 ± 10 km depth.

The above table indicates discrepancies which may be introduced by choosing different depths of compensation.

This station was so chosen that the Pratt-Hayford reduction would give the same anomaly whether we assumed local or regional compensation. As one can see, the differences are not large. Ordinarily, however, if compensation is regional any calculated value of gravity based on local compensation may be in error by as much as ± 0.02 dynes per cm².

The next table indicates errors that might be introduced by not correcting for deviations of density distribution from the standard column.

TABLE II

Apparent anomalies introduced	dynes/cm²
Diabase sill 20 km diameter at 1 km depth (density difference from standard +0.3)	
5 km thick	+0.040
2 " "	+0.018
1 " "	+0.010
0.5 " "	+0.005
0.25 " "	+0.003
Diabase sill 40 km diameter at 5 km depth 5 km thick	+0.032
Sedimentary layer at surface 20 km diameter (density difference from standard −0.3)	
2 km thick	−0.021
1 " "	−0.012
0.5 " "	−0.005

The next table indicates the gravity anomalies that would be obtained by introducing some of the heterogeneities listed in Table II into the column used in Table I.

Thus an isostatically balanced column may have a plus or minus anomaly of as much as 0.030 dynes per cm². Similarly an uncompensated column 350 meters elevation may have a zero gravity anomaly giving us *pseudo-isostasy*.

TABLE III

	Gravity anomalies			
	Normal	*E.*	*F.*	*G.*
A.	−0.020	+0.008	−0.002	−0.041
B.	−0.004	+0.028	+0.014	−0.025
C.	−0.016	+0.016	+0.002	−0.037
D.	−0.009	+0.023	+0.009	−0.030

A, B, C, D are as given in Table I.
E—diabase laccolith 20 km rad., 5 km thick at 5 km depth.
F—diabase sill 20 km rad., 2 km thick at 1 km depth.
G—Deficient density due to light sediments.

Corrections for variations of density from the standard column chosen will have to be made by methods similar to those used by oil geologists in plotting subterranean strata. The depth and method of compensation must be obtained by a statistical method. However, we see that until we can correct for heterogeneity of columns such an analysis must still remain somewhat doubtful.

Thus we see that two stations in which columnar heterogeneities of density have not been corrected for could have a gravity anomaly difference of as much as 0.07 dynes/cm² and yet both be isostatically compensated; hence columnar heterogeneities of density must be corrected for before gravity anomaly discrepancies existing in the second decimal place can be depended upon.

IV. GEOTHERMS. *A. C. Lane*

In Nature, May 9, 1878, Vol. 18, p. 42, there was published a report of a lecture by Sir George Airy, revised by him. In this he summed up the conclusions of over 50 years. One paragraph closes with this sentence: "The form of the earth is not such as would be taken by a solid structure but such as would be taken by a fluid mass with solids floating upon it." This is a theory of isostasy. The next paragraph begins "In the second part of his address Sir George Airy referred to what is known about the temperatures." Again in 1851, Osmond Fisher published his book "Physics of the Earth's Crust" and after Chapters 14 and 15 on the "revelations of the plumb line" and

of the pendulum, he goes on to the "revelations of the thermometer." This paper on temperature followed that of Goranson on isostasy. Thus history repeats itself.

The sources of the earth's heat are (1) environmental, or (2) inherent.

(1) The environmental is mainly from the sun. In that we can not neglect the atmosphere. The temperature drops from 5 to 6°F. per thousand feet over Washington. If we had less atmosphere, as in Mars, there would be a climate more like that of the high mountains of the earth. On the other hand, if we had more atmosphere, we should have a warmer climate at its base. If the sun, or the earth itself, was so much hotter that the present ocean of say, 2000 fathoms depth, was evaporated, the pressure at the base would be 400 times more than at present. The bottom of that atmosphere might have water at the critical temperature 374°C. since the pressure would be above the critical pressure 217.5 atmospheres. There is, therefore, one important error in Lord Kelvin's calculations of the age of the earth in that he assumes the temperature of the earth to have suddenly dropped from a constant temperature of 7000°F. to 0°F. Suppose it had been at 374°C. for a long time, how far down would the wave of change of heat have penetrated in a given time? What is the effect of a a wave of change of temperature going down from the surface?

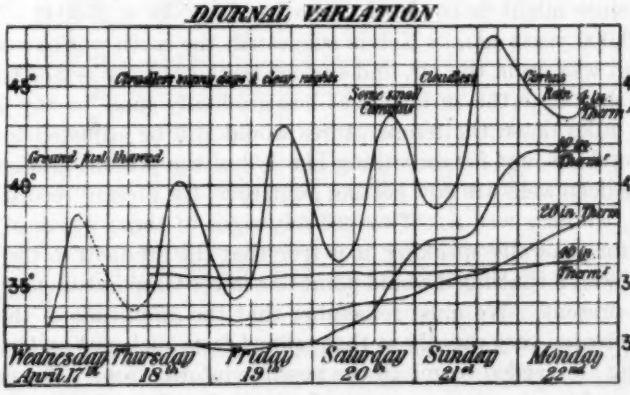

Fig. 1. Callendar's observations of soil temperatures at Montreal, *Trans. Roy. Soc. Can.*, 1895, p. 15, showing the observations for six days with four thermometers at various depths.

To illustrate this, it is well to begin with the daily wave of temperature. One of the most thorough studies has been made by Callendar and McCloud at Montreal. (Fig. 1.) Numerous other studies are avail-

able (by Tamura in Japan, by myself in Michigan, by Spence, Ångstrom, etc.). At Montreal the daily heat wave on the 4 inch thermometer which was hottest about 6 o'clock in the afternoon, shows about 2 o'clock in the morning, Sunday, on the 10-inch thermometer and is barely visible at greater depths. Now the mathematical treatment of all these heat problems shows that in heat problems or any diffusion where the rate of change in time is proportional to the acceleration of change in space (which seems natural, since you would expect no change in time if as much was lost in one direction as was gained in another) the temperature depends upon a function of X (distance) divided by $a\sqrt{t}$, where t is the time, so that if a daily heat wave is reduced to 1/10 or its heat wave in 2 feet, the yearly heat wave will be reduced by the same amount and will similarly be retarded in ($\sqrt{365} = 19$) $\times$ 2 ft. or 38 ft., so that a well whose water is from some 38 ft. below may well be coldest in summer. If there is a heat wave which has occurred during the last 10,000 years and previous to that there was ice for a long time, the effect of that heat wave should (other things being equal) be felt 100 times 38, 3800 ft. down. It is thus desirable to test carefully, as Van Orstrand is doing, the temperatures of wells, especially under some of the ancient lakes. For instance, if Lake Lahontan was cold water 4900 years ago, the effect of the warming of the soil since might be confined to the first $70 \times 38 = 2560$ ft. Thus, if 25,000,000 years ago, as Kelvin suggested, the earth was so hot that the ocean was up in the air, and surface temperature was 374°C., and if since that time the surface heat has gradually sunk to say 24°C., only 38 miles down the temperatures would still be adjusted to 374°. This would account for something like 1/3 of the present gradient.

As the deep wells are now getting numerous, the present record being 9629 ft., we may well ask if we see any signs of these waves of heat. We do, for generally speaking, it is true that the gradient in the lower part of a well if continued would point to a lower temperature at surface than at present. We must remember that the amplitude of the heat wave decreases very rapidly so that refined tests such as Van Orstrand is making are needed to get all the possible information. The general tendency of temperature curves to flatten going down is illustrated in Figure 2 from Van Orstrand and is widely characteristic.

We must not forget, however, that there is another factor to consider —namely, the diffusivity. A blanket is a poor conductor so there may be a great difference of temperature on two sides. The better the conductor, the harder it is to keep up a difference of temperature and the more heat must flow through. In diffusion of heat, however,

another factor comes in, in the capacity of the material to absorb heat itself. The amount it can send on is only the surplus after it has itself absorbed some. So that the diffusivity is the conductivity divided by its heat capacity per unit volume. On the whole, near the surface the strata are porous and less compacted. We should expect them to be poorer conductors and that the rate of increase of temperature would be relatively high. As a matter of fact, however, as Van Orstrand says, it is usually the other way. This increases the probability that a warming up of the world's climate has taken place during the last few thousand years which has produced a flatter gradient for the first 3000 ft. or 1000 meters.

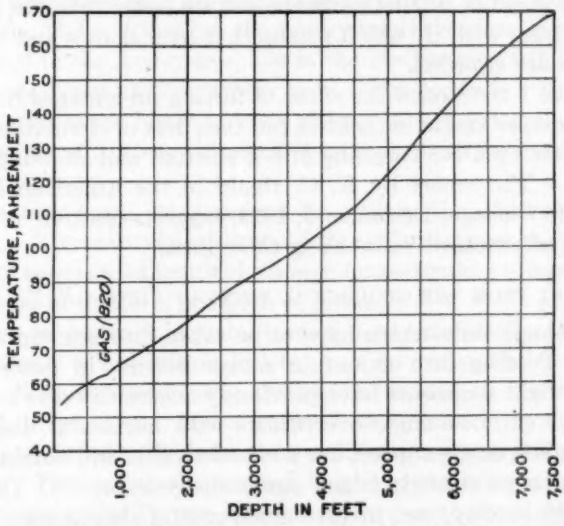

Fig. 2. Temperatures of the Ligonier deep well, Fairmont, W Va., showing the penetration of heat since the last ice age down to about 3500 feet. (After C. E. VAN ORSTRAND.)

Omitting various chemical factors considered by Bischoff in his prize essay, such as oxidation, hydration, and the generation of oil from carbides or by bacterial action, we will consider the effect of radioactive disintegration. Granite is the most radioactive of ordinary rocks, although the range is enormous—three times as much in some granites as in others. The average given by Holmes is 1.3 millionths of a calorie per second per c. c. If the outflow of earth heat is from a gradient of 30°C. per kilometer and the conductivity is .008, the

outflow would be 2.4 millionths of a calorie per second, equivalent to that generated by a column of 18 kilometers of granite. If there is more than that generated, it could not escape by a peaceful steady flow but by occasional spasmodic outbursts like the steam from a tea kettle as suggested by Joly. This would fit in very well with the theories of Schaller and Hess and Kirsch of long time and repeated activity in the formation of pegmatites. We have also the radio-activity of the underlying basalt to account for, but this is only 5/13 to 36/130 of that of the granite. We can see from this the importance of knowledge of what the flow of heat really is and the importance of studies of differing conductivities and diffusivities in the rocks. I have not mentioned the effect of cutting down the surface faster than the geotherms can keep pace with it, which means that over domes and arches the geotherms are crowded.

Nor have I mentioned the effect of forcing up material from below, as in salt domes and in batholiths, nor the effect of circulation of radioactive water up a fault leaking into a stratum and affecting the temperatures. The report by K. C. Heald to the American Petroleum Institute in Chicago, December 5, 1929, suggests research of scientific as well as of practical value along these lines.

V. ISOSTASY FROM THE GEOLOGICAL POINT OF VIEW. *R. T. Chamberlin*

The geologic facts which have to be taken into account are as follows: (1) Folding into mountain ranges occurs, in general, where weak and light sediments have previously accumulated to exceptional thickness. (2) Low-angle overthrusts with horizontal displacement up to 20 miles, or even more, are a characteristic and dominant structure of the more strongly folded mountain systems. (3) The folding, faulting, schistosity, etc., involve much crustal shortening—200 miles for the Appalachian belt (Keith), 125–185 miles for the Alps (Heim), etc. (4) The horizontal squeezing of broad belts of the earth's surface into deformed strips of half their former width forces material either upward, or downward, or both. (5) Resistance is presumably easiest upward. In the surface portion which we see there has been more upward folding than downward. Taking the Briceville, Tennessee, folio as typical of the western flank of the Appalachians, the border thrust fault sharply separates the intricately deformed Appalachian Mountain belt from the flat-lying Pennsylvanian strata of the Cumberland Plateau to the west. The horizontal strata of the Cumberland Plateau (or top of the sedimentary column there) provide a datum plane above which to figure the relative movement. The

deformed mountain region has been folded and faulted upward with respect to this. More recently, since passing through a near-baselevel stage, both the deformed mountain belt and the undeformed plateau belt have been uplifted bodily. (6) There may, in addition, have been some downward movement of material in the deeper reaches of deformed belts far below the surface. Heim has urged this strongly for the Alps.

The key note of isostasy is a working toward equilibrium. It is not a process which upsets equilibrium, but one which restores equilibrium. The light and heavy columns of the isostatic conception tend to become more and more equal in specific gravity, and consequently height, as time goes on. In short, the system by itself, runs down. Relief on the globe, following the purely isostatic view, should have been greatest in the Archeozoic, less in the Proterozoic, still less in the Paleozoic, and least at the present time. Yet today is a time of exceptional relief in the history of the globe. Not denying the validity of the principle of isostasy, it seems clear that it can not be the dominating principle.

Isostatic forces are incompetent to explain the observed crustal shortening. The observed folding and thrust faulting are not primarily the result of isostatic adjustment, though forces tending toward adjustment must be in the total equation of forces.

Operating on a purely isostatic basis a mountain block cut down to baselevel should rise (isostatically) to $\frac{8}{11}$ of its original height (Bowie, Lawson). With the progress of erosion, the mountain column should keep rising, theoretically, and a tremendous amount of erosion would be required to get it finally down to a condition of comparative stability (baselevel). Estimating the average height of the reconstructed Appalachian folds of the Tyrone-Harrisburg section at about 16,000 feet above the Kittatinny peneplain, I calculate that this mountain belt would have to be cut down about 81,000 feet, on a $\frac{8}{11}$ basis, to bring it to baselevel. Yet it has only been cut down about 16,000 feet.

Lawson and others have emphasized the fact that the initiation of a geosyncline must be due to some other cause than loading alone. A plain on which sediments are deposited can be depressed, by loading alone, less than two thirds as much as the thickness of the sediments. The fore-deeps paralleling mountain ranges and chains of islands are striking topographic depressions below the adjacent oceanic floors. They are ready to receive sediment, but not much sediment has yet reached them. In general they show a deficiency of mass.

Following the removal of the Pleistocene ice-sheets, the areas most deeply buried have risen with respect to sea-level. The rise seems to have been greatest where the ice was thickest. This appears to be the best geologic evidence for the importance of the isostatic principle. Explained in part by elastic yield, it appears to require also transfer of material at considerable depth.

Tests to decide between four different types of isostatic compensation have shown that, so far as satisfying the equations is concerned, it makes little difference how the compensation is distributed, or accomplished, so long as its effect is centered at about 35 miles below the surface. The methods are not discriminative, and it is clear that the geodetic evidence does not indicate how the compensation takes place.

What we know of the rocks of the lithosphere indicates great heterogeneity. Myriads of intrusions of different specific gravity cut the earth shell. The present isostatic picture is geometric, not geologic.

The geologic picture of the yielding of the lithosphere is partly by solid rock movement under differential stress and partly by liquid flow of magmas. Each is a means of accommodation to stress, but their behavior is different, and the possibilities are various. Isostatic equilibrium, thus established, would be by complex, heterogeneous compensation. I believe that compensation takes place in some places and at some times in one way; at other places and at other times in other ways; and that a definite depth of compensation is not a physical entity. No uniform depth, nor any single type of compensation, seems consistent with the complex, variable factors involved. We are familiar with the simple concept for the use of mathematics, but we need a complex, composite one for the actual earth, whose many variables transcend the powers of mathematics.

David White has brought out the fact that the gravity anomalies, if one takes the average of a sufficiently large number of determinations for a geologic province, bear a general relation to the density of the rocks in the outermost mile or two of the area. On the other hand, the present anomaly charts of the United States and Canada show an absolute disregard for the topography, the geologic structure, and the kinds of rock of the areas considered. The surprising discrepancy between these two methods of study seems to indicate that the positive or negative sign of the anomaly for any given station has no necessary relation to the regional geology. It may be determined by local peculiarities of density. But for a large number of stations, the preponderance of either positive or negative anomalies does show relationship to the regional geology.

Over large portions of the interior of North America there is little topographic relief to compensate. Compensation is not very significant there, and alternative hypotheses may work equally well. Isostasy is indeterminate from data obtained at low stations. Can we be certain, then, that peneplains are in a state of isostatic adjustment, or is that largely an inference from the fact that mountainous tracts show considerable compensation? In any case, the real test would seem to lie in the limited regions of high relief.

Young mountains, like the Alps, show a marked deficiency of density, and at the same time a moderate excess of mass. An analysis of the mountain-building processes shows why this should be so. (1) The horizontal squeezing of belts of thick, light sediments into strips of less than half their former width greatly thickens the cover of light sedimentary rocks in the mountain belt. As these light rocks close to the surface are nearest the geodetic station, they exert, on the principle of the inverse square of the distance, a preponderating influence on the gravity determinations. (2) Large batholithic intrusions characteristically appear in the cores of folded ranges in a late stage of the deformation. If magmatic differentiation takes place to any great extent, the lighter differentiates will be in the upper portion of the igneous mass. Even though the total weight of the column may be unaffected by this process, these lighter rocks close to the surface will cause the geodetic instruments to give lower gravity values than would otherwise be the case. This will make the excess of mass appear less, though actually it may not be. (3) The squeezing up of mountain folds and the rise of fault slices imposes an added burden upon the supporting lithosphere beneath. Added weight presses downward on the isostatic principle. Hence the lower portion of the deformed wedge mass may be forced downward, while the upper part protrudes upward as mountains. Heim, Kossmat, Born, Lawson, and others believe in a downward movement of the deeper portion of the deformed mass. The denser rock material in the lower portion of the deformed mass may be both depressed more deeply beneath the surface, and also perhaps forced out from the deformed belt beneath the immediately adjoining tracts. If the latter be so, it affords a possible picture of the nature of the accommodation between the lower portion of a deformed mountain wedge-block and the main mass of the lithosphere beneath. At the same time that the denser, deep-seated rocks are depressed and forced outward below, lighter surficial rocks are crowded into the mountain column above. The result is that the mountain mass becomes a lesser burden to be supported than

would otherwise be the case. No division of the lithosphere into a crustal and sub-crustal portion enters into the concept. It is all solid lithosphere, except for local occurrences of magmas. (4) During an orogenic revolution, erosion is actively at work, steadily reducing the excess mass of the rising mountain chains.

If this analysis of the operating geologic processes be valid, we see how geosynclinal strata may be folded and faulted into mountain ranges, exhibiting the familiar observed phenomena, and yet at the same time satisfying the gravity determinations. We see a harmonization of geologic facts and geodetic determinations, so far as deformed mountain ranges are concerned. But all of these processes together, for a considerable time at least, fail to prevent the development of some excess mass in the mountainous belt. The excess mass is supported by the strength of the earth.

If folded mountain chains were formed by the forces tending toward isostatic equilibrium, departures from adjustment should be greatest immediately before the mountains were built. Making the mountains should restore equilibrium, and the adjustment should be most nearly achieved, and most perfect, just as the forces have been spent in completing the mountains. Just the reverse, however, appears to be the case. Recently folded mountains are particularly out of adjustment. Hence we are forced to conclude that mountain folding is primarily independent of isostasy and in direct opposition to it. The mountains are formed in spite of isostasy. Isostatic forces, however, are all in the whole equation of forces, and play their appropriate part, but that part is secondary and subordinate. They are not the fundamental active forces which do things; their function is to restore balance. Isostasy works in opposition to mountain folding; it works in opposition to erosion. When things are doing, or done, it tends to restore equilibrium. Limited thus, to its proper sphere, it is an important principle and should be used as such.

PALEOBOTANY.—*A new* Pterophyllum *from the Shinarump conglomerate in Utah.*[1] EDWARD W. BERRY, Johns Hopkins University.

The remains of terrestrial plants are so infrequent and so poorly preserved in the continental Triassic of the southwest that the discovery of material sufficiently complete to be identified with certainty deserves to be recorded. It was collected by Mr. Arthur A. Baker during the field season of 1928 and may be described as follows:

[1] Published by permission of the Director, U. S. Geological Survey. Received August 1, 1930.

Pterophyllum bakeri Berry, n. sp.

Fronds oblong in youth, somewhat obovate in outline at maturity, of variable size, preserved for lengths of 20 centimeters and varying in maximum width from 2.5 to 9 centimeters. Rachis stout. Pinnules inserted at the sides, closely spaced, subopposite to alternate, united by the whole base, which is neither narrowed nor decurrent, linear in form, with truncated tips rounded at the corners. The pinnules are for the most part nearly straight and symmetrical, but they may curve upward slightly in their distal halves; their angle of divergence from the rachis varies with their position on the frond, the median ones in the most complete specimen collected are at angles approaching 90°, but they become more ascending toward the apex and terminate in an odd-pinnate pinnule. There doubtless was considerable variation from frond to frond with respect to the attitude of the pinnules, since in some specimens they appear uniformly ascending at angles of about 70°. They appear to have been rigid and coriaceous. The veins appear simple and parallel but are not especially clear, and some may bifurcate near their origin, although the undiminished width of the pinnule renders this unlikely. The veins are thin and from 14 to 21 per pinnule.

One fragment shows short, less truncate proximal pinnules less than a centimeter in length, and 6 centimeters higher up they are 3 centimeters long. The following measurements will give an adequate idea of the proportions of the pinnules: Distal, 2.5x0.5 centimeters; median maximal, 1x4.5 centimeters; proximal, 3.5x10 millimeters. It appears to me that all of the specimens collected at this outcrop belong to a single species.

The species is considered new and is named for the collector, Arthur A. Baker. It comes from the top of the Shinarump conglomerate near the southwest corner of sec. 26, T. 41 S., R. 12 E., on the east side of Nokai Creek, Utah.

The material is abundant but somewhat fragmentary and is in the form of impressions with the frond substance entirely oxidized away. It shows considerable variation in size, length, and attitude of the pinnules but is believed to represent a single species. Two specimens showing the extremes of size are figured, and I have reconstructed an entire frond from the various parts illustrated by the various fragmentary specimens.

The various so-called genera of cycadophytes based upon frond characters are not especially precise, nor are authors agreed regarding their limits. *Pterophyllum*, to which the present species is referred, is most likely to be confused with *Zamites* or *Nilssonia*. In *Zamites* the pinnules are not truncate tipped, and they are contracted at the base which is attached to the upper surface of the rachis. In *Nilssonia* the pinnules are short and truncate, but the frond may be entire or split up into segments of unequal width, and the lamina of the two sides meets in the center of the upper side of the rachis to which they are attached. In *Pterophyllum* the pinnules are of uniform width, are usually elongate, and are attached to the sides of the rachis.

No described American species of *Pterophyllum* are especially close

to the present species, although it is possible that some very incomplete pinnules from the Jelm formation ("Popo Agie beds") of Fremont County, Wyoming, which I have recorded as *Pterophyllum?* sp.[2] might represent it. These show neither habit of attachment or tips and have somewhat fewer veins per pinnule and might equally well represent *Zamites* or *Pterophyllum*.

Fig. 1. *Pterophyllum bakeri*, natural size

Newberry described a *Pterophyllum* from the Triassic of Los Bronces, Sonora, Mexico, under the two names of *fragile*[3] and *delicatulum*,

[2] BERRY, EDWARD W., Jour. Geology, **32**: 494, fig. 3, 1924.
[3] In *Macomb's Explor. Exped.*, p. 144, pl. 6, figs. 6, 6a, 1876.

Fig. 2. *Pterophyllum bakeri*, reduced 1/8

which is somewhat similar to the present species. It is said to be of a delicate texture whereas the present form is coriaceous, the rachis is also more slender, although the two specimens which Newberry figures differ considerably in this and other respects, and his Figure 6a is more similar to the Utah form. The material is fragmentary and poorly preserved, and it is impossible to arrive at any satisfactory conclusions concerning it.

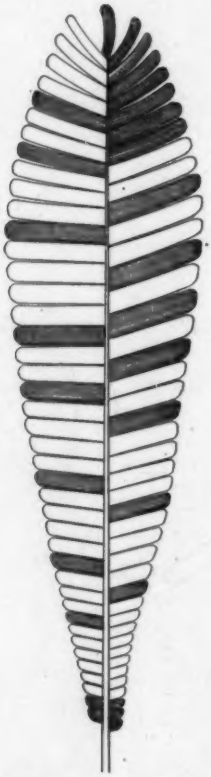

Fig. 3. Reconstruction of *Pterophyllum bakeri*

The genus is abundantly represented in the Triassic of Europe and other parts of the world, and several European species, such as *P. meriani* Brongniart, *P. longifolium* Brongniart, *P. jaegeri* Brongniart, and *P. brevipenne* Kurr, are abundant and have been discussed by numerous students during the last 100 years.

The first of these is probably a young frond of one of the others, as Schimper long ago suggested, and the others show a great amount of variation. *P. jaegeri* is distinctly different from the Utah form, but certain Swiss specimens referred to *P. longifolium*, e.g. one figured by Heer,[4] are exceedingly like the Utah species. The chief differences are the more oblong frond outline, the slightly narrower and more openly spaced pinnules, the fewer veins per pinnule, and the longer proximal pinnules of the Swiss specimen. I consider the American form specifically distinct, but the resemblance is great. *P. longifolium* is a Keuper species.

The genus appears in considerable abundance in the later Paleozoic, becoming cosmopolitan and reaching its zenith in the Rhaetic, continuing abundant through the Jurassic and dying out in the mid-Cretaceous.

SCIENTIFIC NOTES AND NEWS

W. J. HUMPHREYS of the U. S. Weather Bureau has received the following letter from Sir JOSEPH LARMOR, relative to the account of the *Philosophical Society of Washington through a Thousand Meetings* which appeared in this JOURNAL for July 19, 1930: "I have been more than delighted to receive the handsome set of portraits of the heroes of the Washington Academy [Philosophical Society], to not a few of whom I can apply the Tennysonian phrase 'the great Ulysses whom I knew.' The only things approaching it in interest that I possess are a Vienna set of 30 years ago in honour of Victor von Lang and the Berlin account of the German Physical Society in *Wissenschaften*. I was interested to see if I could identify you, also by the predominantly Anglo-Saxon type, if I may so guess, of those days."

L. W. CURRIER has been appointed Assistant Geologist in the United States Geological Survey and has been assigned to work in the Metalliferous Section.

JOSIAH BRIDGE has been appointed Paleontologist in the United States Geological Survey, with headquarters at the National Museum.

LEE T. RICHARDSON has been appointed Junior Chemist in the United States Geological Survey.

FRANK M. WEIDA, Assistant Professor of mathematics at Lehigh University, has been appointed Associate Professor of mathematics at The George Washington University. Dr. Weida is a specialist in mathematical statistics, actuarial theory, and practical mathematical analysis.

RAYMOND JOHN SEEGER has been appointed Assistant Professor of physics at The George Washington University.

[4] HEER, OSWALD, *Flora Fossilis Helvetiae*, pl. 33, fig. 1, 1876.

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